

Geochronology Discuss., referee comment RC2
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Comment on gchron-2021-5

Anonymous Referee #2

Referee comment on "Erosion rates in a wet, temperate climate derived from rock luminescence techniques" by Rachel K. Smedley et al., Geochronology Discuss., <https://doi.org/10.5194/gchron-2021-5-RC2>, 2021

General remarks

The paper „Erosion rates in a wet, temperate climate derived from rock luminescence techniques” presents new data for the application of luminescence rock surface techniques. The approach is applied to rock avalanche deposits from Scotland that have previously been dated by terrestrial cosmogenic nuclides to infer regional erosion rates for the last millennia. The study is well structured, well written and generally easy to follow. It presents valuable new results for the emerging topic of luminescence rock surface dating and erosion rate modelling that are highly needed to better understand the limitations and the potential of the technique. Innovative methodological aspects of the study are the use of a MET-post-IR-IRSL protocol to provide internal quality criteria for the selection of samples with appropriate lithology to adequately record light penetration into the rock surface.

There are several inconsistencies in the paper with regard to the presentation of the data. In particular numbers presented in the main text, the tables and figures do not always match (I will provide details below). This must be revised prior to publication.

Another irritating aspect that needs clarification is the model fit for the unknown age samples in Figure 7. It seems that the inferred model does not really fit the measured data for most of the measured signals. This might indicate inadequate values for μ and/or σ_{phi} and the authors should comment on that.

Apart from that I have only minor comments.

Specific comments

Lines 79-80: Please add a reference for the insights on the exposure history.

Lines 85-93: The shape and position of the bleaching front is also influenced by dose accumulation during exposure. Although this term is irrelevant for most samples, the authors should include a short explanation why they think it is not necessary to address dosing in their setting. It is also confusing that dose rates are considered in the methods section, while they are not part of equation (1). This is confusing and needs clarification.

Lines 104-107: How do you consider temporal variability of μ ? With your approach you rather account for spatial variability of light attenuation between different surfaces and in different depth levels of a surface.

Lines 176-177: Please provide details regarding internal dose rate assessment here. What internal potassium contents were used? How exactly has sample grain size been determined?

Line 190: This should be "successively" instead of "simultaneously".

Line 196: Please provide the number of cores that were used per sample.

Line 202: "...were in line with previous measurements of IRSL signals"

Lines 219-220: How exactly was grain size determined with the microscope? Did you use a software or were grains measured manually? For the latter, how many grains were measured per slice?

Line 235: Which values for μ and σ_{phi} have been used for the fits in Figure 4? I assume you used the parameters presented in section 5.2? If so, I would suggest to change the order of sections 5.1 and 5.2.

Lines 236-237: The numbers given for the depths of the IR50 bleaching front do not agree with the modelled fits in figure 4. Please clarify.

Lines 265-268: Here dose rates are considered, although the term for dose accumulation

is not part of equation (1). So either it was not equation (1) that was used for fitting, or the dose rate information is not needed. Please clarify.

Line 282: The values of Sohbaty et al. (2012) are for quartz signals, thus for a different wave length spectrum. Since different wave lengths are attenuated differently, I would suggest to compare with other feldspar studies.

Lines 282-286: Please provide the numbers for μ and σ_{phi} directly in the text.

Lines 288-292: The unit of the ages should be a instead of a^{-1} . Also the numbers do not match those in table 3.

Lines 306-309: Please provide numbers for erosion rates in the text.

Line 317: Based on the modelled fits shown in figure 7 (red lines), the fit seems not to match the measured data for most of the signals, which indeed indicates that the parameters might be inaccurate. This should be explored in more detail.

Lines 327-329: This assumption does not really make sense in my opinion. Post-IR225 signals need shorter wave lengths than IR50 and post-IR150 signals to be reset. But the attenuation of shorter wave lengths in rocks tends to be stronger than that of longer wave lengths (cf. Ou et al., 2018).

Line 343: I think it should be "exploited" or similar instead of "inferred"?

Figure 1: Please also mark the locations of the road cuts and provide photographs of the road cut sampling sites.

Figure 3: Why are the datasets for grain size and RGB different? Were the analysis performed on different slices?

Figure 4: I would suggest to mention in the figure caption that core 3 of Road 2 was not considered for fitting. Also, information regarding the inferred model (which μ , which σ_{phi} , reference) should be provided.

Figure 5: I recommend to add the inferred model fits that are shown in figure 7 also here. In figure 7 it seems that the model fits (red lines) do not match the measured data for most signals, but it is hard to judge since the panels are rather small. Figure 5 would allow for a much better evaluation.

Figure 6: The unit for the ages should be a instead of a^{-1} .

Figure 7: It seems that the model (red lines) in C, G, h and I does not really fit the data. Could you please comment why this might be the case? Please also provide more information on the likelihoods (where do these come from) and the forbidden zone (how do I recognize it in the figure).

Figure S4a: The signals are only identical for sample ROAD 01 (1 a reference sample).